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SCIENCE

NEW YORK, MARCH 17, 1893.

WHERE IS THE LITRE?—A MODERN SCIENTIFIC PUZZLE-PICTURE.

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IN *Engineering News* of Oct. 20, 1892, appeared an article on Fuel-Gas Values, in which I gave a table entitled "Some Metric Constants," designed to show the variations of value to be found in the text-books even with regard to so fundamental a matter as the volume of the litre. The publication of this table has caused me to receive a letter of protest from my friend, Mr. Latimer Clark, F.R.S., who, as all the world knows, takes rank among the foremost living authorities on the subject of weights and measures; his "Dictionary of Metric and other Useful Measures" being a permanent masterpiece. This letter contains much that is interesting to the scientific world, as will be seen by the following quotations which include all the material passages:—

"I have looked over the varying list of values and it is not very difficult to account for the discrepancies. Many of them have taken the values as defined by Act of Parliament, and as published by the Board of Trade. But all the world has known for years past that this valuation is very far wrong, and therefore the more careful writers have endeavored to correct the error as far as they were able by using the best results they could obtain or hear of. Some of them, however, are not quite so easily explained (S A. Ford, for example).

"For the past thirty years no scientific writer or worker has used the Board of Trade official value of the cubic inch of water, viz., 252.458 grains. This is the simple cause of the discrepancies you point out. You have been a little hard on me in the matter, and your article would certainly lead any one to suppose that I had given three different values for the litre, which is very far from being the case. After the book was all printed ready for issue, the new Board of Trade measurements came out and I rewrote and reprinted a great part of it in order to make it conform to the new legal definition of the Board of Trade. Up to September, 1891, I had always assumed the cube decimetre and litre to be identical. . . . At page 57 I call especial attention to the change, in the footnote, and again in the article 'Water,' at page 90, and I give there a table of the volumes of the litre and cube decimetre. Then, again, at page 103, I give a special note on the capacity of the litre. I beg you to read these with care, for it is evident that you have read hastily and have never put your back into the question. If you had read carefully you would have found abundant warning against confounding the litre with the cube decimetre. They are practically the same, and can be differentiated only by means of the most costly apparatus used by the most skilful physicists and with extraordinary precautions; but then you were writing from a scientific point of view and you ought to have read carefully.

"Then in reference to the 'cube inches into litres,' page 47. You ignore the six places of decimals given in the first column, and pass on to the subsidiary column of reciprocals where only two are given, and by some process you expand them into five places of decimals, some of which are, of course, sure to be wrong. Strangely enough, too, while going to this trouble, you fail to notice that on this line and the one above it ('into cube decimetres') the two figures are given differently, viz., 61.04 and 61.0270. This would certainly have caught your eye if you had been really studying the question, but I fancy you were more intent upon writing a rattling article for the press.

"I hope you will find some opportunity of correcting the impression that my book is not trustworthy, for it is at the present day the *only* book that gives the English measures correctly.

"I note that in the constants you have adopted, you use 28.3127 as giving the number of 'litres in a cube foot.' I do not quite see what you take this from, but in England the number is 28.3110, while the number of cube decimetres is 28.3153.

"In the United States the metre is by law = 39.37 inches, but in England it is 39.37079 inches. From the latest measurements, however, the U. S. number is likely to turn out more accurate than the English number."

In order that this letter may be clearly understood it is desirable to quote the published statements to which it refers. These are as follows:—

1. The reference to Mr. Clark's book in my table appeared thus:—

"Authority.	Cu. inches in 1 litre.	Cu. feet in 100 litres.	Litres in 1 Cu. ft.
Dictionary of Metric Measures, by Latimer Clark, F.R.S.	61.0364	3.5322	28.3110
Ditto	61.04	3.5323	28.3093
Ditto (cube decimetre.)	61.0270	3.5316	28.3153"

2. After directing attention to some current arithmetical inaccuracies on the subject of the heat-value of natural gas, I remarked as follows, in the paper concerning which Mr. Clark has written me:—

"Considerations of space forbid my entering at further length into the correction of published errors. Every careful man who has ever consulted a text-book will grimly admit the justice of this remark; even though he may willingly agree with me in sincerely thanking the Trautwines and Haswells and Gmelins and Clarks and Thomsons and Favres and Regnaults and Berthelots, and all the brilliant compilers who have done so much good and worthy work in aiding the progress of knowledge."

3. The foot-note at p. 57 of Mr. Clark's book is:—

"The litre was designed to be the volume of a cube decimetre of water in vacuo at maximum density, but is actually somewhat greater. It is now understood as the volume of one kilogram of water freed from air, at maximum density and weighed in vacuo. It is, therefore, dependent on the dimensions of the kilogram and not of the metre. The litre used in these tables has the capacity above defined; the equivalent weight of water employed is not the kilogram but the actual weight in air (see 'Water')."

4. The article "Water" at page 90 of Mr. Clark's book is:—

"The weight of the cube inch of water at 62° F., used in the following table and throughout the work, is not the old and well-known cubic inch of 252.458 grains, but the newer determination by the Standards Department of the Board of Trade, viz., 1 cubic inch of distilled water, freed from air, at 62° F., weighed in air against brass weights, barom. 30 inch = 252.28599 grains. This measure has already been legalized. It is distinguished by the date 1890. The old weight of the cubic inch was legalized by Act of Parliament in 1824, and when used it is distinguished by that date.

"The gramme of water is very commonly considered identical with the cubic centimetre, and the kilogram is similarly taken as equivalent to the cubic decimetre or litre, but these relations are only true when they are weighed in vacuo and at maximum density, 4° C. The litre of water (1 kilogram in vacuo at 4° C.) when weighed practically, that is, against brass weights in air, barom., 30 inches, loses 16.5 grains, owing to displacement of air, and then weighs at 4° C. only 998.93 grammes instead of 1,000. The difference is, of course, greater at ordinary temperatures. In addition to this, the kilogram, and therefore the litre, is supposed to be intrinsically heavier than the cubic decimetre of water in vacuo by about 120 milligrams or 1.85 grains, owing

to slight errors in the original determination. Dr. O. J. Broch (International Committee of Weights and Measures, Annales de Chimie et Physique, tome X., February, 1887) remarks that the centimetre employed in fixing the dimensions of the original kilogram of water would appear to have been $\frac{1}{33000}$ longer than the present standards. The freedom from air was also a point which was not regarded at that period.

Weight of Distilled Water, Free from Air, Weighed against Brass Weights, Barom. 30 Inch.

Volumes.	In grains		In grammes	
	62° F.	4° C.	62° F.	4° C.
Cube inch of 1824	252 458	252.741	16.3591	16.3773
" " " 1890	252.286	252.568	16.3479	16.3662
" " weighed in vacuo	252.556	252.839	16.3654	16 3837
Cube foot (62.2786 lbs.), 1890	435950.2	436458.2	28249.11	28280.73
Gallon (10 lbs)	70000.	70 78.3	4535.93	4541.01
Litre (1 Kilog. in vacuo)	15398.6	15415.8	997.814	998 930
Cube decimetre	15396.3	15413.5	997.662	998.779
Cube centimetre	15.3963	15.4135	.9977	.9988

"Water increases in volume from its maximum density at about 4° C. (or 39.2° F.) to that at 16⅔° C. in the ratio of 1 to 1.001120 (log. 0.0004863). At the same time its density or specific gravity diminishes in the same ratio, or as 1 to .998881 (log. $\bar{1}.9995137$). These figures are taken from government reports. The true maximum density is said to be at 3.945° C., but 4° C. is the accepted standard."

5. The "Note on the capacity of the litre" at p. 103 of Mr. Clark's book is:—

"The relation between the British and Metric measures of capacity depends on the value which we assign to the litre.

"This value may be obtained as follows. The litre is the volume of one kilogram of water at 4° C. in vacuo. If we suppose the litre of water to be raised in temperature to 62° F., its weight will not change, but its volume will have expanded to 1.00112 litres (Chaney, Proc. Roy. Soc., No. 294, Sept., 1890). If 1.00112 litres at 62° F. weigh 1 kilogram, or 15432.35 grains, then 1 litre will weigh $\frac{15432.35}{1.00112} = 15415.08$ grains.

"If we bring this new litre into the air, and weigh it against brass or bronze weights, it will sustain a further loss of weight, due to the buoyancy of the air. This will amount to 16.491 grains, as described below, and the weight of the litre in air at 62° F., Bar., 30 in., will then be:—

	15415.08 grains
Less loss by weighing in air	16.49
Weight of the litre in London at 62° F.	15398.6 grains

"The original litre has, therefore, lost 17.25 grains by its expansion in volume, and 16.49 grains by the buoyancy of the air acting on it and the weights which counterbalance it. Having thus ascertained that the litre of water at 62° F. weighs 15398.6 grains, and the cubic inch 252.286 grains, we easily find that the litre contains 61.0364 cubic inches.

"The loss of weight in air is thus calculated. Mr. H. J. Chaney, warden of the standards, who has recently re-determined the weight of the cubic inch of water (Chaney, Proc. Roy. Soc., No. 294, Sept., 1890), finds that one cubic inch of ordinary air, containing an average proportion of moisture and carbonic acid, weighs in London .3077 grains at normal pressure and temperature. 61.0364 cubic inches, therefore, weigh 18.781 grains. The weights, if of bronze, have a specific gravity of 8.4, and if of brass of about 8. Taking a mean density of 8.2 we get $\frac{18.78}{8.2} = 2.29$ grains due to the displacement of air by the brass weights. Deducting the 2.29 grains from 18.78, the displacement due to the water, we get 16.49 grains, the value used above.

"It would not be possible to measure the litre or the cubic decimetre strictly as defined by the French Statutes, for they prescribe that the water shall be weighed at 4° C. in measuring vessels which are to be correct at 0° C. There is a similar anomaly in the definition of the American gallon."

6. The values given at p. 47 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube inches into cube decimeters,	.016386	61.0270	$\bar{2}.21448$
" " " litres,	.016384	61.04	$\bar{2}.21441$

7. The values given at p. 57 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Litres into cube feet,	.035322	28.3110	$\bar{2}.54804$
" " inches	61.0364	——	1.78559
" cube centimetres, or gram., water at 4° C.	1000	——	3.00000

8. The values given at p. 32 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube feet into cube metres,	.02832	35.3166	$\bar{2}.45202$
" " " decimeters,	28.3153	——	1.45202
" " litres or kilogs. of water 4° C.	28.3110	——	1.45196

9. The values given at p. 24 of Mr. Clark's book are:—

	Multiply.	Divide.	Log
Cube decimetres into litres,	1	——	0.00000
" " " cube feet,	.03532	28.3153	$\bar{2}$ 54798
" " " " inches,	61.027	——	1.78552

10. The values given at p. 61 of Mr. Clark's book are:—

	Multiply.	Divide.	Log.
Cube metres into cube feet,	35.31658	——	1.54798
" " " " inches,	61027.05	——	4 78553

The foregoing quotations, together with Mr. Clark's letter, form a very excellent puzzle-picture, in which, presumably, the litre is somewhere to be found. Before, however, I adventure upon the search, let me clear away four small clouds that might otherwise befog the expedition.

First, Mr. Clark is mistaken in imagining that I had not read his book carefully and that I "confounded the litre with the cube decimetre." No. 1 of the foregoing quotations shows that in my table I specifically drew attention to the distinction between the two measures in question.

Second, Mr. Clark is mistaken in imagining that, with reference to the values given at p. 47 of his book, I "failed to notice that on this line and the one above it ('into cube decimetres') the two figures are given differently, viz., 61.04 and 61.0270." No. 1 of the foregoing quotations shows that the two figures in question must have "caught my eye": for I duly included both of them in my table and took care to show that one referred to the litre and the other to the cube decimetre.

Third, Mr. Clark is mistaken in imagining that my "article would certainly lead any one to suppose that (he) had given three different values for the litre." Any careful reader of the table (vide quotation No. 1) would see that I cite Mr. Clark as having given two different values for the litre and a third value for the cube decimetre, which is, in very deed, the case.

Fourth, quotation No. 2 shows that I took some pains to preclude any impression that Mr. Clark's book is not trustworthy.

Coming now to the main question, let us commence our investigation by summarizing the statements in Mr. Clark's book and letter as to the various measures that all come under the common denomination of "litre." They are as follows:—

A.—"LITRES" PROPER.

1. "Litre = 1 cube decimeter, or $\frac{1}{1000}$ cube metre, very nearly. The volume of 1 kilogram water at 4° C. . . . It is now understood as the volume of 1 kilogram of water, freed from air, at maximum density, and weighed in vacuo" (p. 57). The accepted temperature of maximum density is 4° C. (p. 91). The weight of 1 Kilog. of distilled water, free from air, in vacuo at 4° C., is 15432.35 grains (p. 103); and the weight of 1 cubic inch (of 1890) of water under the same condition is 252.839 grains (p. 91). Hence the volume of the standard litre is $\frac{15432.35}{252.839} = 61.036272$ cubic inches.

2. If a standard litre of distilled water, free from air, be weighed in London against brass weights in air at 62° F., barom. 30 in., the result will be 15398.6 grains (p. 103); and the weight of 1 cubic inch (of 1890) of water under the same conditions is 252.286 grains. Hence the volume of the "London" litre (of 1890) is $\frac{15398.6}{252.286} = 61.036284$ cubic inches.

3. The volume of the "London" litre (of 1824) is $\frac{15398.6}{252.458} =$ 60.9947 cubic inches.

4. The value of $\frac{15398.6}{252.286}$ adopted by Mr. Clark is 61.0364.

5. The weight of 1 kilogram of distilled water, free from air, in vacuo, at 62° F., is $\frac{15432.35}{1.00112} = 15415.0851$ grains, and the loss by weighing in air is 16.491 grains. The weight of the litre in London at 62° F., barom. 30 in., is thus 15398.594 grains; and this, divided by 252.286, gives 61.03626 cubic inches as the volume of the London litre (of 1890).

6. On the basis of 61.03626 cubic inches per litre, the number of litres in 1 cubic foot is $\frac{1728}{61.03626} = 28.31104$.

7. On the basis of 61.0364 cubic inches per litre, the number of litres in 1 cubic foot is 28.310975.

8. The value of $\frac{1728}{\text{Cu. in. per litre}}$ adopted by Mr. Clark is 28.3110.

B.—CUBE DECIMETRES.

1. The English metre is 39.37079 inches. Hence the English cube decimetre is 61.027051 cubic inches.

2. The weight of 1 cubic decimetre of distilled water, free from air, weighed in air against brass weights, at 4° C., bar. 30 in., is 15413.5 grains; and the weight of 1 cubic inch under similar conditions is 252.568 grains. Hence the volume of the standard cubic decimetre (English 1890) is $\frac{15413.5}{252.568} = 61.027129$ cubic inches.

3. The value of $(3.937079)^3$, adopted by Mr. Clark, is 61.0270 cubic inches.

4. The U. S. metre is 39.37 inches. Hence the U. S. cube decimetre is 61.023377953 cubic inches.

5. On the basis of 61.027051 cu. inches per cube decimetre, the number of cube decimetres in 1 cubic foot is $\frac{1728}{61.027051} =$ 28.31531.

6. The value of $\frac{1728}{\text{cu. in. per cu. dec.}}$, adopted by Mr. Clark, is 28.3153.

7. The number of U. S. cube decimetres per cubic foot is $\frac{1728}{61.023377953} = 28.31702$.

C.—CONVERSION VALUES.

1. "Cube centimetres into cube decimetres (litres)"—divide by 1000 (p. 17).

2. Cube centimetres "into litres"—divide by 1000.05 (p. 17).

3. Cube decimetres "into litres"—multiply by 1 (p. 24).

4. "Kilogram = 1000 grammes = 1 litre, or 1 cube decimetre water, 4° C. Miller, in 1856, found the kilogram = 15432.349 grains in vacuo. It was originally intended to be the weight of a cubic decimetre of water at maximum density in vacuo. It is now a definite mass of platinum and is slightly heavier than the cubic decimetre of water" (p. 50).

5. "Cube metres or steres (= 1000 litres very nearly) into litres"—multiply by 1000 (p. 61).

"Cube metres or steres into cube decimetres"—multiply by 1000 (p. 61).

"Cube metres or steres into cube feet"—multiply by 35.31658 (p. 61).

"Cube metres or steres into cube inches"—multiply by 61027.05 (p. 61).

6. "Kilograms (or litres) of water into cube inches"—multiply by 61.170 (p. 92).

"Cube feet of water into litres, 62° F."—multiply by 28.311 (p. 93).

"Cube feet of water into kilograms 62° F."—multiply by 28.249 (p. 93).

From this summary it will be seen that Mr. Clark's book and letter present us with quite an extended range of choice for the value of a litre, viz.:—

Standard litre (1890)	61.036272	cu. inches.
" decimetre	61.027051	" "
" " (weighed in air)	61.027129	" "
London litre (1890)	61.03626	" "
" " (1824)	60.9947	" "
Clark " (1890)	61.036284	" "
" " "	61.0364	" "
" decimetre	61.0270	" "
" "	61 02705	" "
U. S. "	61.023377953	" "
"kilogram" (in vacuo, 4° C., 1890)	61.104666	" "
" (in air 62° F.)	61.170	" "

and, in addition to these, I may quote the following from Table 1 of the before-mentioned article on "Fuel-Gas Values," viz.:—

Authority.	Cu. ins. in litre.
U. S. Dispensatory, 16th ed.	61.0280
G. Gore, LL.D, F.R.S.	61.024
Professor V. B. Lewes, F.C.S.	61.024
Professor J. D Everett, F.R.S.	61.022
Trautwine (said to be U. S. Standard)	61.0254
	61.024425
Haswell (said to be by Act of Congress)	61.022
"	61.02524
Gmelin	61.0267
W. Crookes, F.R S.	61.02709
Thomson and Tait	61.02432
S. A Ford	64.99008

The suggestion made by Mr. Clark that these discrepancies may for the most part be explained by the difference between the 1824 and 1890 standards is obviously insufficient if the difference he refers to be that of the cube inch value; for as the 1824 value is 60.9947 it clearly was not adopted by the authorities above quoted. Some other explanation is, therefore, required; and as so consummate an authority as Mr. Clark appears unable to advance one, I may perhaps be allowed to hint that the cause of the varying values is to be found in sheer laxity of calculation. I know that so commonplace a theory is rather shocking, and I duly blush as I advance it; but, really, when I find Mr. Clark himself deliberately adopting the value 61 0364 as the quotient of $\frac{15398.6}{252.286}$ and adopting it as the basis of his book, whereas the true quotient is 61.036284, or, if four places of decimals be used, 61.0363, I may plead for pardon with some assurance of the same being accorded. The example here cited is even still more to the point; for the value 15398.6 is adopted by Mr. Clark as the result of the calculation $\frac{15432.35}{1.00112} - 16.491$, whereas the true result is 15398.594 and this divided by 252.286 gives 61.03626.

But let it not be imagined that I make these remarks in any fault-finding or critical spirit. I am too conscious of my own short-comings to be willing to sit in the seat of judgment. In the before-mentioned table, for example, I derived Mr. Clark's second value of "cubic feet in 100 litres" from his figure of 61.04 cubic inches per litre. The calculation was, of course, $\frac{6104}{1728} = 3\ 5324$; and yet, when I corrected the proof of the article, I inadvertently allowed the value to appear as 3.5323. So I must ask my scientific brethren to understand that my observations are not intended as any disparagement of the "Dictionary of Metric Measures" or as casting any adverse reflection upon the other text-books I have quoted, all of which I regard as admirable examples of scientific work and as trustworthy as reasonable mortals can expect them to be.

And so we come back once more to our question, Where, after all, is the litre? Our puzzle-picture turns out to be of a kaleidoscopic variety and appears in a different aspect to every

observer. In spite of the much-lauded simplicity of metric measures, we find that the "litre," has as many meanings as the "pound," that it is addicted to the reprehensible habit of impersonating its fellow-measures, that the virtue of its mother centimetre is open to grave suspicion, and that its own constancy is no better than it should be. What, then, are we to do? The answer to this question appears to me to be plain enough, and, indeed, constitutes the object I have had in view in originating and pursuing the discussion. The lesson of the litre teaches us the importance of a duty that is too often neglected, namely, the prefixing (or affixing) to every scientific paper, or treatise, a table, or other statement, setting forth the values assigned to the constants employed by the author. If this be done, it matters not one whit whether the values chosen are in accordance with the most rigorous determinations or depart therefrom. If any reader choose to attach different values he can then do so; whereas under the present system of every man being a hidden law unto himself, the perusal of a scientific work is not a process to which the phrase "emollit mores" can be justly applied.

Another lesson that we may learn from the litre is the futility of a besetting scientific sin, namely, the Affectation of Accuracy. The owner of that holy and hosannád thing, the "scientific conscience," is apt to deem himself "not as other men" and smiles complacently at the thought that he has expended long years and a fortune in determining, for example, that a cubic inch of water under certain conditions weighs 252.28599 rather than 252.28598 grains. And yet the same gentleman will, from his lofty pedestal of physics, look down with much pity, if not with absolute contempt, upon the equally conscientious entomologist who (vide *Nature*, Nov. 17, 1892) wears away a thinking and working lifetime in determining whether a certain insect walks upon more than three legs at once. The results of the most refined investigations are but approximations to the truth, after all; and in most cases of scientific work an approximation sufficiently close to the truth to serve all practically useful purposes can be arrived at easily and expeditiously. Accuracy, therefore, may often be, in the true sense of the term, excessive, even if intrinsically trustworthy; but when we consider that what appears accurate to one generation is regarded as inaccurate by the next, we must surely deem it but a poor thing to boast of. Take, for example, Mr. Clark's confession that up to September, 1891, he "had always assumed the cube decimetre and litre to be identical"; a confession which, coming from so distinguished an authority, is tantamount to a demonstration that most other physicists shared the same erroneous impression, and therefore that the much-vaunted accuracy of modern work in physical science has not existed to the full extent claimed. And yet we all know that the work has been really magnificent and solid, both in its contributions to the world's store of knowledge and in its advancement of the welfare of mankind. This certainly teaches us that reasonable care in scientific measurement is sufficient care, and that extreme care is, by the very nature of things, doomed to fail of its object.

A PRESUMABLY NEW FACT RELATIVE TO THE CEDAR WAXWING (*AMPELIS CEDRORUM*), WITH REMARKS UPON THE IMPORTANCE OF A THOROUGH KNOWLEDGE OF FIRST PLUMAGES.

BY EDWIN M. HASBROUCK, WASHINGTON, D. C.

IT is considered by every one that the individual waxwing possessing wax tips on both secondaries and rectrices is in the highest development of plumage, while a high development of plumage in any species whatever is usually accorded to the older birds.

Coues states that, "Specimens apparently mature and full-feathered frequently lack the wax-tips"; that "their normal appearance is unknown," and that "birds in the earliest known plumage may possess one or more." Beyond this little appears to be known.

In a somewhat extensive series of waxwings in the National Museum, in my own and other collections, appendages on the wings were developed in forty-five, fifteen displayed the ornaments on both wings and tail, while the remainder, apparently adult birds, were entirely unadorned. (It might be well to state that the females as well as the males possess these tips, although less frequently, while some specimens examined showed the ornaments on both wings and tail.) Now, the natural conclusion from this would be that those birds possessing wing-tips only were older than those having none at all, while the fifteen on which both wings and tail were adorned were even older and were in the highest perfection of plumage. This is disproved by the fact that four birds of the year still in the striated plumage, taken in August, September, and October, respectively, display very distinct tips on the secondaries; and if on the secondaries at this early age when older birds possess none at all, why should they not also appear on the tail-feathers? The supposition of older birds only being adorned being disposed of, the question arises, When do these horny appendages appear? and on this I am able to throw considerable light.

It was in the summer of 1884 that I was spending a month at Port Byron, N. Y., when I ran across a nest of the waxwing, containing four young, every one of which had the wax tips on tail and wings perfectly developed. These birds were nearly fledged, although unable to fly, and I had good opportunity to observe them. Not being interested in collecting birds at that time they were not preserved, a circumstance to be regretted, but the full import of these appendages being developed in nestlings was appreciated.

The following table for the calendar year shows the conditions of specimens examined. So regularly and so nearly is it completely filled that it is evident that an examination of a larger series would undoubtedly fill the gaps.[1]

Month.	Wings.	Both.	None.
Jan.	♂		♂ ♀
Feb.	♂	♂	♂ ♀
Mar.	♂ ♀		♀
Apr.	♂ ♀	♂	♂
May	♂ ♀	♂	♂ ♀
June	♀	♀	♂ ♀
July	♂	♂	♂
Aug.	♂ ♀ ♂ im	♂	♂ ♀
Sept.	♂ ♂ im		♂ ♀
Oct.	? ♂ im		♀
Nov.	♂		♂ ♀
Dec.	♂		♂

With this evidence it is apparent that these handsome ornaments are by no means a sign of age, but are, on the contrary, a purely individual development, appearing sometimes in their highest perfection in the nestling, while in an adult they may be entirely absent or barely beginning to appear; or again, appearing a few months after attaining first plumage, to go through a regular course of growth and development. Inasmuch as an individual with wax on both tail and wings is exceedingly rare, and the August and September birds are just beginning to acquire the tips it would be interesting to know just how often this development in the nest occurs, and this is published mainly with the hope of eliciting further information on the subject, and of prompting those in the field to be on the watch the coming season.

The importance of thus studying the first plumages cannot be too highly estimated, for not until comparatively recent years has a careful and thorough study of the life-history of each and

[1] In this table an attempt has been made to show merely that both sexes are adorned for each month in the respective columns. In a number of instances several individuals were found for each.

every one of our birds been deemed of any great importance by ornithologists. Of late, owing to the discovery of numerous errors that had crept into our nomenclature, careful attention has been paid to a species from the time of its advent into the world to a period when beyond all doubt it has reached its maturity. To the collector who accumulates a series, it is only too apparent how great is the difference between individuals, and that his series is not complete until each and every phase of plumage from various widely separated localities is represented.

Late in the season, while the full migration is at its height, a bird is secured which for the life of him he cannot name; in vain he searches the literature, compares specimens, and puzzles and worries only to find it at last an old acquaintance flitting under new colors. I have in mind a young man who, although not an accomplished ornithologist, ought to have known better, and who essayed to publish a list of the birds of the locality in which he lived. One winter he secured a bird entirely unknown to him, and in his dilemma sent it to the Smithsonian for identification; on its return the label bore: "American Goldfinch in winter plumage." This may be a little foreign to the subject but it shows how necessary was a thorough knowledge of the life-history of the species. Nor was it so very long ago that the "Gray Eagle," which for years was accorded specific rank, was found to be but an immature phase of *Haliæetus leucocephalus*, while *Oidemia perspicillata trowbridgii* was shown to be but a seasonal variation of *perspicillata* proper. Even to this day it appears not to be generally known that the Golden Eagle takes from three to five years to acquire its full plumage; that the Bald Eagle attains his highest plumage at the age of three, the various intermediate stages being known as the Black Eagle, Gray Eagle, etc., and that the Little Blue Heron is pure white the first year, mottled and variegated with blue in every conceivable manner the second, and attains the perfection of its plumage only at the age of three; yet such are the facts. These are but isolated cases, while any day may bring about the unification of some two forms which at present are considered at least sub-specifically distinct.

BOILING-POINT AND RADIUS OF MOLECULAR FORCE.

BY T. PROCTOR HALL, CLARK UNIVERSITY, WORCESTER, MASS.

WHEN a bubble of its own vapor exists in a liquid the pressure, P, upon it is the sum of the three pressures:—

A, due to the air,

W, due to the water above the bubble,

C, due to molecular cohesion.

Let us suppose, for convenience, that the bubble is so close to the surface that W may be neglected. When the radius, r, of the bubble is large compared with R, the radius of molecular force (i. e., the distance at which a molecule ceases to exert a sensible attraction), the pressure, C, over its diametral plane is equal to the surface tension, T, across the circumference. That is to say,

$$\pi r^2 C = 2\pi r T$$
$$\text{or } C = 2T/r.$$

Then $P = A + 2T/r$; and the temperature must be such that P is balanced by the molecular energy of the vapor if the bubble is to continue to exist. As r increases $2T/r$ decreases, and for bubbles of ordinary size may be neglected in comparison with A, the ordinary pressure of the air. Hence the lowest possible boiling point of a liquid is such that the vapor pressure is just sufficient to overbalance the air pressure. But at one or more points in the liquid the temperature must be very much higher, or no bubbles of vapor could be formed. This condition occurs whenever a liquid is boiled in a rough vessel.

If a liquid be uniformly heated no bubbles can be formed until the temperature is such that $P = A + C$ for the whole liquid when the bubbles are first formed. When this point is reached bubbles are formed everywhere, the pressure upon them decreases very rapidly as they increase in size, and the liquid explodes. The explosion point, like the boiling-point, depends in part upon the pressure of the air, but has a definite lower limit when $A = 0$.

Unfortunately the value of C in terms of the surface tension cannot be calculated directly for the explosion point; but a probable value may be found as follows:—

When a U-shaped wire in an inverted position is drawn up from a liquid, in many cases a film is formed between the wire and the liquid surface. For a pure liquid the thickness, k, of the film is nearly constant, though it varies greatly in some solutions. The film has a measurable tension, $2T$, across every linear centimetre on its surface. In other words, a force of $2T$ is drawing apart, against the forces of cohesion, a liquid whose section is $(K \times 1)$ square centimetre. It seems probable, therefore, that the liquid will give way at every point when the expansive force opposing C becomes $2T/k$ on each square centimetre; so that at the explosion point

$$P = A + 2T/k.$$

In 1861, Dufour (Comptes Rendus, 52, p. 986) succeeded in heating water to 175° C., and chloroform (which boils at 61°) to 98°, under ordinary air pressure, without explosion. Assuming that these are approximately the explosion points of water and chloroform, we may calculate, from the known values of the surface tensions and the vapor pressures at these temperatures, that the value of k for water is 123 $\mu\mu$ (1,000,000 $\mu\mu$ = 1 mm.), and for chloroform 200 $\mu\mu$. From a solitary case, which may be only a coincidence, it would be rash to generalize; yet it is interesting to notice that the ratio of these two values of k is almost exactly the ratio of the molecular diameters of water and chloroform.

Now R, the radius of molecular force, is known to lie somewhere between k and $k/2$ (see Jour. Chem. Soc., 1888, p. 222). Hence, if the preceding equality of ratios be found to hold for other liquids we shall have the theorem that the radius of molecular force is proportional to the diameter of the molecules.

Quincke (Pogg. Ann., 137, p. 402, 1869), by a method likely to give results a little too low, measured R and found for water 54 $\mu\mu$, a value which is in close accord with that given above.

The experimental determination of the explosion points of different liquids requires no complicated apparatus and would have considerable scientific interest. I make the suggestion for the use of any one who has time and inclination for research without the advantages of a well-equipped laboratory.

DR. GEORGE VASEY.

DR. GEORGE VASEY, Botanist of the Department of Agriculture, died, in the City of Washington, March 4, 1893. He was born on Feb. 28, 1822, at Scarborough, Yorkshire, England, and came to America when a child. He graduated from Berkshire Medical College at Pittsfield, Mass., in 1848, and settled in Illinois, where he practised his profession for twenty years. He was appointed Botanist to the Department of Agriculture in April, 1872, and held the position until his death. As Botanist to the Department he was Honorary Curator of Botany in the U. S. National Museum, and it is largely from his efforts that the present herbarium of over 23,000 species has been accumulated and arranged. His main work has been upon grasses, and among other papers he has printed "Descriptive Catalogue of Native Forest Trees of the U. S.," 1876; "Grasses of the United States: A Synopsis of the Tribes with Descriptions of the Genera," 1883; "Agricultural Grasses of the United States," 1884; "Descriptive Catalogue of the Grasses of the United States," 1885; "Report of Investigations of Grasses of the Arid Regions," two parts, 1886–87; "Grasses of the South," 1887; "Agricultural Grasses and Forage Plants of the United States," a revised edition, with 114 plates of "Agricultural Grasses," 1889; "Illustrations of North American Grasses; Vol. I., Grasses of the Southwest," 100 plates with descriptions, 1891; Vol. II., Part 1 of the same, "Grasses of the Pacific Slope and Alaska," 1892; "Monograph of the Grasses of the United States and British America" (Vol. III., No. 1, Contributions from U. S. National Herbarium) 1892.

He was a delegate from the Department of Agriculture and the Smithsonian Institution to the Botanical Congress in Genoa, last September, returning immediately after the adjournment of the congress. He was a member of the Biological and Geographical Societies of Washington, and a Fellow of the American Association for the Advancement of Science. He was taken sick on Feb. 28, and died after a short illness on the morning of March 4, of constriction of the bowels.

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CALIFORNIA PICTOGRAPHS AND HIEROGLYPHICS.

BY MRS. THEODORE H. HITTELL, SAN FRANCISCO, CAL.

THE study and investigation of the works of the earliest inhabitants of a country is now a science in itself, and is receiving more and more attention in all parts of the world.

Too little has heretofore been done in California, Alaska, Nevada, and Arizona to investigate, gather up, and preserve the relics and works of the prehistoric races which inhabited these western territories, and as there is now but little left, that little should without delay be carefully sought out and put in such shape as to remain a permanent possession. Of much, on account of our own carelessness we have been despoiled, and much that yet remains has been more or less defaced and injured.

Government, as well as scientific societies, should look to the preservation of what remains of the structures, tools, utensils, and weapons of the aborigines, and by all means endeavor to gather together and preserve by photographs the cipher writings which are yet to be found and which, year after year, by the corroding hand of time and the more destructive hand of ruthless vandalism, are becoming more and more defaced and ruined.

The cipher writings yet to be found from Alaska to Arizona, if carefully gathered and studied, might enable us to learn many very important facts concerning the customs of the redskins and their early history.

In the Sierra Nevada Mountains, near the so-called Summit Soda Springs, about fourteen miles south of Donner Lake and at an elevation of about 6,000 feet above the level of the ocean, the attention of tourists is attracted by numerous inscriptions incised in the rocks.

The most prominent, and the most inviting of attention of these, are those cut in the granite rocks, about a hundred feet high, which stand nearly isolated on the right and on the left of the headwaters of the North Fork of the American River.

The stream there is almost a little torrent and dashes over the rocks in cascades and from there it plunges into and through a mountain gorge towards the lower level far below.

To a person standing near the fountain-head of the river, on the rocks against which it chafes and which it is gradually but surely wearing away, and who takes note and truly appreciates the grandeur of the scenery, there comes a feeling of awe and reverence. It elevates the soul and calls forth a spirit akin to religious worship.

It was here in this sublime region that an unknown people left pictographs on the rocks pertaining doubtless to their history and religion. The seasons of centuries since then have come and gone; the snows of uncounted winters have covered them; succeeding springs and summers unnumbered have decked the mountains with yearly verdure, and the river has been rushing on and on and cutting its bed deeper and deeper. All this we know; but we know nothing of those who wrote these ciphers on the monumental rocks. They have long since passed away.

Only with the help of science and long study and comparison, can we hope to gain an inkling of the meaning these ciphers were intended to convey, and add, perhaps, some important facts to the ancient history of California—a subject now so full of interest and becoming daily of more and more interest to the world.

According to the Report of the Bureau of Ethnology at Washington, pictographs of the North American Indians are found at Santa Barbara and San Diego in California, and in Nevada, Arizona, Oregon, Idaho, and Utah.

In Nevada great numbers of incised characters of various kinds are found on the rocks flanking Walker River. These are waving lines, rings, and what appear to be vegetable, animal, and human forms. Among the copies of pictographs obtained in various portions of the Northwestern States and Territories by Mr. Gilbert, one kind is referred to as being on a block of basalt at Revielle, Nevada, and is mentioned as Shinuma or Moequis.

This suggestion is based upon the general resemblance to drawings found in Arizona, and known to have been made by Mosquis Indians.

In Oregon, numerous boulders and rock escarpments at and near the Dalles of the Columbia River are covered with incised or pecked pictographs. Human figures occur; but other forms predominate. From Lieut. J. H. Simpson's Topographical Bureau Report we take the following: "At the Rio de Zuni, in 1849, we met Mr. Lewis, who had been a trader among the Navajas, and according to his statement had seen inscriptions on a rock on his travels to and fro. He offered to guide us. He led us to a low mound. We went up and found inscriptions of interest, if not of value; and of them some dating so far back as 1606. The rock is since mentioned as Inscription Rock." The following letter, addressed to Lieut. J. H. Simpson, was written by Danatiana Vigil, Secretary of the Province of Santa Fé, on October 19, 1849.

"*Sir:*—The engravings which are sculptured on the rock of Fish Spring, near the Pueblo of Zuni, copies of which you have taken, were made in the epoch to which they refer. I have an indistinct idea of their existence; but, although I have passed the place some three times, I never availed myself of the opportunity to observe them. The other signs or characters noticed are traditional remembrances, by means of which the Indians transmit historical accounts of all their remarkable successes. To discover these sets by themselves is very difficult. Some of the Indians make trifling indications, which divulge, with a great deal of reserve, something of their history, to persons in whom they have entire confidence. The people who inhabited this country before its discovery by the Spaniards were superstitious and worshipped the sun."

Mr. G. K. Gilbert discovered etchings at Oakley Springs, Eastern Arizona, in 1878, relative to which he remarks that an Orabi chief explained them to him and said that the Mosquis make excursions to a locality on the Colorado Chiquito to get salt. On their return they stop at Oakley Springs and each Indian makes a picture on the rock. Each Indian draws his crest or totem, the symbol of his genus. He draws it once, and once only on a visit.

From Alaska to Arizona many inscriptions on rocks are found. Of some of them photographs have been taken. But so far as we know none are as extensive or of such variety and of so ancient a date as those situated near the source of the American River.

These pictographs seemingly resemble and are written in much the same way as the Chinese ciphers where each figure is a word and has a full meaning, and seemingly they should be read from right to left.

Max Müller says, in writing of the American aborigines: "Though the Indians never arrived at the perfection of the Egyptian hieroglyphics, they had a number of symbolic emblems, which were perfectly understood by all their tribes. For instance, power over man is symbolized by a line drawn in the figure from the mouth to the heart. Power, in general, by a head with two horns. A figure with a plant as head and two wings, denotes a doctor skilled in medicine. A tree with human legs, a herbalist. Night is represented by a finely crossed or barred sun, or a circle with human legs. Rain is figured by a

dot or semicircle filled with water and placed on the head. The heavens with three disks of the sun is understood to mean three days' journey; and landing after a voyage is represented by a tortoise. But there is no evidence to show that the Indians of the north ever advanced beyond the rude attempts which we have thus described."

Lord Kingsborough's publication of "The Mexican Hieroglyphics" shows a higher developed intellect among that people and cannot be placed in the same category with those of the aboriginal Indians of the United States. They are colored, written on paper, and are in many respects equal to the hieroglyphic inscriptions of Egypt.

These most interesting books with their colored picture-writings, copies of which are in the possession of our California Academy of Sciences, are worthy of the cost expended on them and the attention given them by scientific men. They give an idea of the true condition of the inhabitants of Mexico before the landing of Cortez. Max Müller says:—

"One of the most important helps towards the deciphering of the hieroglyphics is to be found in certain American books, which, soon after the conquest of Mexico, were written down by natives who had learned the art of alphabetic writing from their conquerors, the Spaniards. Ixtilxochitl, descended from the royal family of Tezcuca, and employed as interpreter by the Spanish Government, wrote the history of his own country from the earliest time to the arrival of Cortez. In writing this history he followed the hieroglyphic paintings as they had been explained to him by the old chroniclers. Some of these very paintings which formed the text-book of the Mexican historian, have been recovered by M. Aubin, and as they helped the historian in writing his history, that history now helps the scholar in deciphering their meaning.

It is with the study of works like that of Ixtilxochitl that American philology ought to begin. They are to the student of American antiquities what Manetho is to the student of Egyptian hieroglyphics or Berosus to the decipherer of the cuneiform inscriptions.

A small part of the hieroglyphics found at the source of the American River, which I have thus described, have been photographed by Mr. Jackson, principal of the Sacramento Art School.

But, for the sake of science, it would be well, it seems to me, to have the whole of the rock-inscriptions photographed and preserved for ethnological and scientific research.

INVOLUNTARY RECOLLECTION.

BY JAS. W. DONALDSON, ELLENVILLE, ULSTER COUNTY, N.Y.

IF one will but organize himself into a society for "psychical research" and, cultivating a habit of introspection, observe carefully even his own mental processes, he will find much to interest and confound him.

And perhaps no other operations of his mind will furnish him with more occasion for thought and investigation or prove more interesting and suggestive than some of the vagaries of *involuntary* recollection.

There are few persons indeed of ordinary intelligence to whom this at times strangely spontaneous habit of memory is not a familiar, recognized experience, exciting more or less their wonder and speculation.

For example, we can all recall occasions when, though however earnestly engaged with other thoughts, we have all at once awakened to the discovery that we were at the same time unconsciously humming a snatch from some old song, or mentally repeating a fragment of prose or verse learned in our childhood, which we had supposed was long since buried so deep down under the *débris* of years as to be beyond the hope of resurrection; yet there it was, as fresh and vivid as ever, having, with a dash of its old-time irrepressibility and abandon, burst in upon our consciousness again without so much as asking "by your leave."

We may remember, too, that it has often happened that these unexpected visitants were of a character to cause us much discomfort and humiliation, for we have found by sad experience that we may not easily "pluck from memory a rooted sorrow," nor "raze out the hidden troubles of the brain;" and, worse than all, that the "damned spot" will never "out," however frantic and agonizing may be our entreaty. Indeed, it is impressed upon us that, if there be any of our memories which are more perverse and persistent than others, it is the erratic, or disreputable ones, which we have thoughtlessly garnered and forced into unnatural companionship with our graver and better impressions. These will return again and again in spite of us, and it seems, as if with malicious intent, that they often delight in choosing opportunities when it is most to our embarrassment and mortification.

Perhaps we are at a funeral, and have become touched and subdued by the saddening ceremonies, or at church, earnestly engaged with its impressive services, when, all at once, without warning, one of these irreverent sprites of memory, with cap and bells and many a comic antic, breaks in upon our serious mood, and, wantonly disregarding the sanctities of the occasion, makes mouths at its solemnities. Or it may be that sometime when in the midst of a scene of innocent mirth and jollity the ghost of an unavailing remorse, or the shadow of an event in our life full of shame and agony, may suddenly appear to sadden and sober us and dissipate our enjoyment.

The writer recalls an incident in his own experience illustrating the sometimes strange unexpectedness of this phase of recollection.

Many years ago he was moved to memorize certain quaint and amusing verses found in a newspaper. On a March day long after, as he was riding out of Albany, and in a comfortable and complacent mood listlessly gazing out of the car window upon the bedraggled and cast-off garments of a rough and dissipated winter, suddenly these verses, committed over thirty years before, broke in upon his thoughts and began to reel themselves off with the startling abruptness and unmanageable spontaneity of a wayward alarm-clock.

Perhaps it was more than twenty years since they had last occurred to him. He tried in vain to discover what in all that dreary, forbidding landscape, or in the nature of his thoughts, had set this jangling waif of memory agoing, but could not in any way account for it; nothing in his mind seeming to bear the remotest relation to it. Apparently, as if obedient to some unexplained law of periodicity, this disreputable tramp of the brain had, in its vagabond wanderings, rounded its period, and, with an impudent smirk and an affected wail of distress, there it was again, begging, "for Christ's sake," a dole of recognition at the open door of an unwilling and repelling consciousness.

Possibly, if we accept the later, and what seems the more reasonable, conception of consciousness, that is, that it is not all of memory, but merely one of its phases or conditions, and a dependent, unstable one at that, we can the better account for some of these freaks of spontaneous recollection.

It is evident that a normal brain has more or less control over that which shall cross the threshold of consciousness, for we know many persons have the faculty of so absorbing themselves with any certain line of thought as to be seemingly quite oblivious for the time to everything else not pertinent to it.

But, while it may appear that they are generally successful in thus holding the door against a besieging host of interloping and disturbing recollections, yet even they, too, sometimes fail to make the exclusion completely effectual.

Indeed, because of the very intensity of their thinking and their unusual turmoil of brain, they are likely to arouse and quicken other associations having certain constituent elements in common with those entering into the texture of their main thought, and these, too, may sneak into cognition along with the invited guests in spite of their every precaution.

Again, with the majority of persons, "mind wandering" is more or less a besetting infirmity. The spring which holds the door of their consciousness either has a congenital weakness or has become more and more impotent because of disease or approaching senility, and is therefore capable of offering little resistance to any strays of memory which may seek to enter. In fact, so degenerate do some minds become, that consciousness,

once a guarded and sacred preserve, is now a commons or thoroughfare through which any vagrant, motley procession of thoughts may troop at will without let or hindrance.

No doubt, too, this open, unguarded condition of consciousness may come upon us at times simply from our relaxing nervous tension, as when we unharness the will and turn it loose, and lapse generally into a state of mental passivity and listlessness. It is then that, finding the door left ajar, these unbidden recollections oftenest make their intrusive entrance.

Perchance, too, Unconscious Cerebration may take advantage of the situation to display and call attention to some of its remarkable curios, and, abusing the opportunity, lug in with these certain annoying remembrances which we would it had left in undisturbed oblivion.

Perhaps one of the most significant and suggestive revelations that comes to us from a thoughtful observation of these extraordinary phenomena of involuntary recollection, is the abundant proof they furnish us of the unexpected and marvellous tenacity of our impressions.

It is made very manifest that those potential organic conditions which were set up and established in the original process of developing these impressions are still preserved to us intact, and need only the proper excitant or stimulus to revive and rehabilitate them for us again and again.

Being well assured of this, it would seem profitable for us to inquire to what extent, not yet realized, can we, by a deliberate and persistent exercise of the will, control and compel these conditions of revival.

We are all conscious of doing a good deal of recollecting by *voluntary* effort, but it is mostly those ordinary experiences which are comparatively recent and fresh. When it comes to making labored and prolonged effort to restore some elusive and faded image of a remote past, we are easily discouraged, and, even though it be a momentous event in our lives, a vivid and complete recollection of which might save us from dishonor or utter ruin; yet, after making a few hopeless and abortive attempts to remember, we are apt to give up in despair, when, perhaps, had we been fully possessed with an abiding faith in the enduring nature of our impressions and in the possibility of our reviving them, no matter how remotely fixed, we might have hopefully and courageously continued our efforts, even for days or weeks if necessary, until the missing fact was again brought into the fold of consciousness.

Surely, if, as has often happened in human experience, grave accidents or emergencies have resulted in so quickening and rehabilitating certain conditions of the brain as to fully restore to the person recollection of events long supposed to be irretrievably lost, it demonstrates the reasonableness of our employing and confidently relying upon systematic and patient effort to compel the same active and exalted mental conditions to produce the same happy result.

THE ARRANGEMENT AND NUMBER OF EGGS IN THE NEST.

BY DR. MORRIS GIBBS, KALAMAZOO, MICH.

ALL birds have a system or arrangement in depositing their eggs in the nest, and there are very few species, if any, in which some peculiarity is not to be seen, if careful observation is made. Many birds so plainly and invariably show a tendency to a set arrangement that their habit is generally known. It is of these well-known examples that we will speak.

The loon or great northern diver always deposits two eggs. They are almost perfectly elliptical in shape and lie side by side. The eggs are invariably found at over three-fifths of the distance from the front edge of the nest depression, that is, at about two-fifths of the long diameter from the rear end of the elongated hollow or nest proper. From the position of the eggs one can tell how the bird sits on the nest, as we may reason that, with these long-bodied birds, the abdomen, which supplies the direct heat, is well back from the front of the hollow. This theory is verified by watching the incubating bird. The turtle dove, night-hawk, whippoorwill, and common domestic pigeon, each of which lays two eggs at each setting, deposit the eggs side by side, although this arrangement is frequently interfered with in the case of the tame bird, not rarely with the result that one of the eggs does not hatch.

The spotted sandpiper and killdeer plover, and I presume most of the other snipe and plover, lay four eggs at a clutch. The eggs are arranged in the nest, or on the bare ground, with their small ends together, and, as they are pyriform in shape, they join in to perfection. The eggs of the snipe and plover groups are proportionately exceeding large for the size of the bird, and the saving of space by this arrangement undoubtedly answers a purpose. It is impossible to offer a solution to this problem of order at present, unless we may suggest that it is a wise provision of some ruling power, which so ordains the arrangement which best admits of the bird's covering the eggs thoroughly. It is fair to doubt if a sandpiper could cover her four large eggs if they were arranged in any other position besides that in which they are found, with the four smaller ends pointing to a centre. This species has a small body and is not provided with loose, fluffy feathers, so well supplied to many grouse and other birds which lay many eggs. On two occasions the order of the eggs in nests of the spotted sandpiper was broken by us; an egg being turned about with its point presented outward. One of these nests was deserted, perhaps from the interference, but in the other the order was found restored within a day.

Perhaps no bird in America, certainly no other in Michigan, equals the common bob-white or quail in the number of eggs it sets upon. This species not infrequently lays eighteen eggs, and even more are found in one nest, but I can assure the readers that with any other shaped eggs the bob-white could never succeed as a successful setter. I will suggest that my friends with collections at hand compare a set of twenty eggs of the quail with twenty eggs of equal dimensions in longer and shorter diameter of any other species, and observe which lot occupies the smaller space. We may say, for illustration, that the bob-white's egg is triangular, and fits in as no other egg, to my knowledge, can.

With all birds which lay a good-sized clutch, so far as my observations go, the eggs are deposited in almost an exact circular group. The bird must use excellent judgment in thus arranging them, for it is only by this order that they can all be covered properly. Not infrequently when a grouse is startled from her eggs she tumbles one of her treasures from its bed. If the egg is not too far removed, it will almost invariably be found returned to its exact position in the nest within a few hours.

I have been informed that the brown pelicans steal eggs from one another's nests, in order to fill their complements, or at least take possession of those they find lying on the ground and roll them into their nests. Although this does not seem at all likely, for various reasons, I cannot dispute it authoritatively, and, moreover, there were strong proofs that such was the case in many nests that I examined in Florida. These nests, which were near together, often contained four eggs, never more; one to three of which were ready to hatch, the others being fresh, or nearly so. And, again, there would be eggs in the same nest with young over a week old, or young of ages quite ten days variation. But one point was ever observable, the young, or eggs, or both, never exceeded four in number, showing, even if the charge of abduction is proven, that the old birds know their limit.

The cow-blackbird, in imposing its eggs on the care of other birds, not rarely fails in the arrangement of affairs. It is fair to allow that the cow-bird is perfectly able to distinguish its own eggs from those of the blue-bird, chipping-sparrow, and others, which differ radically in size and color from its own speckled, tough-shelled eggs; but I believe it often fails to distinguish its eggs from the quite often similar ones of the chewink and oven-bird. And this failure accounts for its depositing as high as four and five eggs in the nests of the chewink, where there was but one egg of the owner; and again laying four eggs in an oven-bird's nest, which contained no eggs at all of the owner,— both cases undoubtedly oversights, which resulted from its inability to distinguish. It is reasonable to allow that cow-birds have limits as to the number to be deposited, otherwise some unfortunate warbler

or other small bird would be overwhelmed. As it is, the cowbird studies the limits of endurance in its victims and rarely exceeds the bounds. The most eggs I ever found in a nest infested by cow-birds was nine, and the species generally lays only two or three eggs, thus generally keeping the outside limit to six or seven, with the owner's eggs.

THE USE OF THE TERM "CARBOHYDRATES."

BY W. E. STONE, PH.D., PURDUE UNIVERSITY, LAFAYETTE, IND.

IT has frequently happened in the history of chemistry that names and terms have lost their original significance so soon as the knowledge of the bodies to which they were applied has become more extended. "Organic" chemistry is better named the "chemistry of carbon compounds;" the "aromatic" bodies have disappeared in the broader designation of benzine derivatives. In the same way it appears that we have reached, or already passed, a transition stage in the use of the term "carbohydrates." Treatises on chemistry still retain the old definition of the term, while those familiar with recent progress in this field no longer feel themselves restricted to these ancient limits. It is the purpose of this paper to consider the present status of this subject.

Von Lippmann, in his work "Die Zuckerarten und ihre Derivate," adopts Fittig's view that the carbohydrates are derived from the hypothetical heptatomic alcohol $C_6H_7(OH)_7$, which, by loss of water, forms the simple or complex anhydrides, $C_6H_{12}O_6$ or $C_{12}H_{22}O_{11}$, known as sugars. His treatment ignores the existence of any carbohydrate with less than six carbon atoms, although he says that, with the (at that time, 1882) slight knowledge of the constitution of the carbohydrates, it was impossible to regard this definition as final and complete.

In 1888 appeared Tollens' "Handbuch der Kohlenhydrate," in which the definition of "carbohydrates" was limited strictly to the bodies composed of C, H, and O, containing six carbon atoms, or some multiple of six, and H and O in the same proportion in which they are found in water. But already Kiliani had shown that arabinose, which had long been regarded as a true carbohydrate on account of all its reactions, had really the composition $C_5H_{10}O_5$. Moreover, it had already been established that the best known sugars, such as dextrose, levulose, galactose, and arabinose, had the constitution of aldehydes or ketones of the hexatomic, respectively pentatomic, alcohols. In anticipation, therefore, of evident progress along this line, Tollens remarks in his preface that such bodies as arabinose and the impending erythrose might well be regarded as carbohydrates, but he retains the hexatomic nature as a requirement for the "true carbohydrate," and puts all non-conforming but similar bodies under the head of "den Kohlenhydraten nahestehenden Körper."

Up to this time a sort of understanding had prevailed that the carbohydrates were exclusively products of natural forces. It had also been noted that these bodies gave certain reactions, which were also presented as a basis for the classification given.

These reactions, as stated by Tollens, are:—

1. Reduction of alkaline metallic solutions.
2. Rotation of polarized light.
3. Subject to alcoholic fermentation by yeast.
4. Formation of levulinic acid.
5. Formation of characteristic compounds with phenylhydrazin.
6. Certain color reactions.
7. Solubility, either before or after hydrolysis.
8. Decomposition by heat.

All of which hold strictly true for the hexatomic carbohydrates. This classification was probably as liberal as the state of knowledge at that time would justify.

But this classification is evidently arbitrary and ought not to have weight in comparison with any classification based on chemical constitution. If a similar constitution can be proven for a series of bodies, the fact that they respond to certain reactions will only be additional proof of their relationship. Such reactions must, of course, be general in their nature, while special reactions will only serve to characterize individuals. In this way the class of carbohydrates must eventually include only bodies of certain constitution, while the characteristic reactions will be limited to a smaller number of more general application. A similar development has taken place in the manner of classifying the hydrocarbons, alcohols, acids, glycerides, etc.

Of the carbohydrates conforming to the old definition, dextrose, levulose galactose, and mannose are types. They respond to the reactions given and have been found to possess the constitution of ketones or aldehydes of the hexavalent alcohol, $C_6H_{14}O_6$. But we know two bodies of the formula $C_5H_{10}O_5$, arabinose and xylose, which are also aldehyde alcohols, and which give the same reactions as their homologues, with the exception of fermentation and the formation of levulinic acid. Again, we know an aldehyde of the tetratomic alcohol erythrit, called erythrose, of the formula $C_4H_8O_4$, which responds to the same general reactions as its homologues. Glycerose, $C_3H_6O_3$, has also been studied and found to correspond to the others of the series in constitution and general reactions. It is even fermentable with yeast like the regular carbohydrates, which shows this to be an intermittent reaction when applied to an homologous series. Beginning again with the group $C_6H_{12}O_6$, we find that there have been prepared synthetically three other homologues representing aldehydes, respectively of the hept-, oct- and nonatomic alcohols. These also respond to the general reactions given, except that they do not form levulinic acid. Heptose and octose do not ferment, but nonose, with its multiple of three carbon atoms, is fermentable.

It is no argument against the carbohydrate nature of these bodies to say that they do not occur in nature, since two of the hexoses (galactose and mannose) have never been found free, but are only known as derivatives of certain natural products. In this respect they are on precisely the same footing as arabinose, xylose, erythrose, and glycerose.

It appears, therefore, that we have an homologous series of aldehyde or ketone alcohols of the general formula $C_nH_{2n}O_n$ with these common properties: 1° sweet to the taste; 2° optically active; 3° reducing alkaline metallic solutions; 4° yielding with phenylhydrazin characteristic crystallinic compounds. Other reactions, such as great solubility, decomposition by heat, and color reactions, are less characteristic, although possessed in common. Those containing three, or multiples of three, carbon atoms undergo alcoholic fermentation with yeast, and this periodical reaction seems an additional argument for their common nature. Individually they yield, when heated with strong acids, characteristic derivatives; for instance, the pentoses yield furfurol; the hexoses levulinic acid; others have not been carefully studied in this direction.

Following are the members of this homologous series which are known, although several additional isomers are possible:—

Triose, $C_3H_6O_3$.—Glycerose.

Tetrose, $C_4H_8O_4$.—Erythrose.

Pentose, $C_5H_{10}O_5$.—Arabinose, xylose.

Hexose, $C_6H_{12}O_6$.—Dextrose, levulose, galactose, mannose, all in isomeric forms.

Heptose, $C_7H_{14}O_7$.—Heptose.

Octose, $C_8H_{16}O_8$.—Octose.

Nonose, $C_9H_{18}O_9$.—Nonose.

By the definition of carbohydrates, now extant, only the hexoses are included. It is the purpose of this paper to propose the extension of this term to all members of the homologous series, on the basis of a common constitution, viz., as aldehydes or ketones of the normal polyatomic alcohols of the aliphatic series. As characteristic properties of all these, must follow their behavior toward polarized light, toward alkaline metallic solutions, and toward phenylhydrazin.

Such a classification would exclude the bodies of the cellulose group, of which there are many, more or less well defined. But it is not yet evident that they possess a constitutional relation to the bodies under discussion, and have certainly no claim to be classed with the aldehyde or ketone alcohols because convertible into them.

As for the disaccharides of the hexoses, to which belong sucrose, lactose, etc., if it be true, as supposed, that they are anhydrides or ether-like forms of the hexoses, then they are entitled

to a place among carbohydrates as derivatives or modifications of the same.

E. Fischer proposes to apply the name "sugars" to all the members of this homologous series, to which he has lately added the glycol-aldehyde $C_2H_4O_2$ as the simplest possible example. The popular conception of the properties of a sugar are not, however, easily reconciled with the properties of some of these bodies, while "carbohydrates" at least possess some reference to their impirical composition. With regard to glycol-aldehyde, moreover, its optical inactivity would exclude it from the list under the conditions here proposed, although its constitution undoubtedly satisfies the requirements.

ELECTRICAL NOTES.

Variations in Resistance.

IN a recent article in the *Philosophical Magazine* appears a paper by Mr. Fernando Sandford, entitled "A Necessary Modification of Ohm's Law." Why it should have been given this title does not appear, for it nowhere calls in question the law which goes by Ohm's name. A better title would have been "On the Variation of Resistance of a Conductor with Change of the Medium Surrounding It." The facts observed are of interest, though not new, as it has long been known that the resistance of a wire changes when immersed in different gases. Chatelier, for example, found that the resistance of a silver wire changed enormously when immersed in hydrogen gas, and that if left in it for some time its temperature coefficient changed also. Mr. Sanford has extended the list considerably, his experiments, though made with a wire of one metal only, i.e., copper, embrace a great variety of mediums, both liquid and gaseous. That the variation is due to the causes noticed in the experiments of M. Chatelier and not to heating of the conductor, as proposed by some, is probable from the following considerations. The total heat generated in the wire, using the ordinary coefficients of emissivity for polished copper, would not raise the temperature of the wire more than the one ten-thousandth of one degree centigrade, and the increase of resistance from this cause would be inappreciable. But the effect of a thin film on the wire would be far different. It was first pointed out by Mr. Kennelly to the writer that the extremely thin film of tin on electric conductors was sufficient to lower the resistance of moderately small wires as much as five per cent. If we suppose that when a wire is placed in a gas like SO_2 a thin film of a compound of the copper and the gas is formed, only the one twenty-five-thousandth of an inch in thickness, it will account for all the phenomena observed by Mr. Sanford. For, as the wire experimented on was one millimetre in diameter, the formation of a layer $\frac{1}{25000}$ of an inch thick would reduce the cross section of the copper by two-tenths of 1 per cent, and therefore increase the resistance by 0.2 per cent, or nearly the maximum change observed by Mr. Sanford. This thickness of film is not much greater than the thickness of the films which cause the iridescent colors on steel, being about three to five times as thick; so that we see that the slightest action of the gases on the surface of a wire would change the resistance quite appreciably, and on exposure to air the wire would recover itself again. It should be added, moreover, that such films would not necessarily be visible.

An easy way of settling the question would be to use wires of different diameters. With a wire whose diameter was .0035, or No. 40 B.W.G., and which is furnished for commercial purposes, the resistance should vary as much as one and a half per cent, while with a wire one centimetre in diameter it should be inappreciable.

R. A. F.

A JOINT meeting of the Scientific Alliance of New York, in memory of Professor John Strong Newberry, will be held at Columbia College, Monday evening, March 27, 1893, at 8 o'clock. An address will be given by Professor H. L. Fairchild, "A Memoir of Professor John Strong Newberry." Remarks will be made by others, and a number of letters regarding Professor Newberry will be read.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Does the Ether Absorb Light?

WHETHER or not light is absorbed in any degree by the ether through which it passes has been argued a good many times, and to-day is not settled on any experimental basis. That it is not so absorbed to any considerable degree is evident from the light from such distant stars that reaches us. From theoretical considerations some have concluded that many more stars would probably be seen by us if in some way their light was not stopped by the ether, and that the midnight sky would or should be brighter than it really is.

In all the treatments of the subject which I happen to have seen, there is one important element which has not been considered at all, and to me it seems as if that one would account for the limit to the number of stars we see without assuming that the ether possesses the ability to transform energy within itself, which would be the case if the energy of waves like light waves were changed into any other kind of energy not capable of affecting our eyes This fact is, that, in order to see, some energy is needful. I mean that there must be some limit to the amplitude of the vibratory movement beyond which we could not see, simply because the energy of the wave is insufficient; so that no matter what the intrinsic brightness of a given light may be, if it be far enough removed from an observer it will cease to be visible, simply because the energy of the waves is too small to excite the sensation. As the energy of such radiant energy on unit area varies inversely as the square of the distance, and as the amplitude of the vibrations at the initiating atoms or molecules can at best not exceed the diameter of the atoms or molecules, the extreme minuteness of the amplitude at the distance of the fixed stars from us shows how exceedingly delicate is the eye for perceiving it at all. The enormous frequency of the waves gives them a degree of energy they could not otherwise have; but if there were no amplitude there would be no energy, and it is to be conceived that if space be illimitable and the number of stars be infinite, yet with eyes constituted like ours only the light of stars within a limited space would be visible, and such optical data would give no reason for holding that what could be seen was the whole, nor for the conclusion that the light from more distant stars was absorbed by the medium through which it was distributed.

The photographic work done in this field testifies to the same conclusion when we are presented with the image of a star which had never been seen. The photographic plate acts cumulatively and if one minute's exposure is not enough, take ten minutes or ten hours, but the eye cannot so act; if one cannot see an object in a second, he can see it no better by continued looking. I conclude, therefore, that we have no evidence that the ether absorbs any of the energy of the ether waves.

A. E. DOLBEAR.

Tuft's College, Mass., March 9.

Natural Selection at Fault.

IN your issue of Feb. 17, Mr. Richard Lees replies to the rather misleading article of Mr. J. W. Slater in your issue of Jan. 20, and takes, it appears to me, the right view of the case as regards the Felidæ, but misses it when he attempts to account for the hen's cackle. No one reason will account for the latter. Frequently the hen that is a member of a large barn-yard flock may be observed cackling at the top of her voice prior to the laying of the egg, and it has been my observation that in 9 cases out of 10 this is due to the fact that she has found a usurper in her nest in the person of another hen engaged in egg laying. Close observation, covering many years, leads me to think that the cackling after the egg is laid has nothing whatever to do with nest-disclosure or nest-hiding, but is simply a notification to the cock of the flock that the important task of the day is accomplished.

There is no time at which the hen is so susceptible to the gallant attentions of her liege lord as just at the end of this cackling period. I have frequently observed this of our barnyard fowls, of guinea hens, both domestic and in the wild state, and of pea-fowls. In my opinion, the cackle is intended to notify the male bird of the Barkiss-like condition of his mate.

As to the case of the cat tribe, it is so common to see a mother cat in the country bring field-mice, young rabbits, moles, or ground squirrels in to her kittens and watch their playful antics with them, that the conclusions arrived at by Mr. Lees are irresistible. This winter an intelligent house-cat, on a farm where I have been studying winter life in field and woods, led me some distance to where several grain-ricks had stood during the fall. I soon saw that she wanted me to turn over the fence-rail floor that still remained there, that she might capture the field-mice living beneath. This I did, while Tabby caught four mice in quick succession. The first one she gulped down at a rapid rate, the second she played with a little while, the third she played with much longer and, half-devoured, left to her eldest son, a full-grown Tom who had accompanied us, and the fourth she barely wounded and also turned over to his tender mercies. In a word, while hunger was a dormant passion, she quickly devoured her prey, after that her instinctive disposition to practise and keep perfect the arts whereby such elusive game is captured was paramount.

Mr. Slater is in error in thinking that a comparatively few now possess the power to "wag the ear." This power is common among the West Indian half-breeds and the Maya and other derivatives of Mexico and Central America, and many whites have the power who hardly realize the fact. It is not uncommon to observe this if one will suddenly say to a companion, "What was that noise?" If Mr. Slater will say this in a semi-startled way, he will notice that in no inconsiderable number of cases there will be a slight instinctive movement of the muscles in question, more or less pronounced. Nor is the ear that Darwin illustrates in his "Descent of Man" as being allied to the pointed type belonging to our Simian relatives as uncommon as many may imagine. It is my observation that this peculiarity of the fold in question is oftenest to be observed in women, and in many of these cases the persistence of the wisdom teeth is also a characteristic. I have in mind two cases of this sort, one of a man, the other of a woman, both residents of one of our leading cities, and their social and intellectual forces. The latter is a remarkable reversion to an earlier type, in ear, in teeth, in length of arm, in painless childbirth, in flexibility of hand-joints, and in other marked characteristics. It appears to me that the ear, like the vermiform appendix, the suspension of the viscera, the position of the orifice to the bladder, and the unprotected condition of certain main arteries, is yet in a transitional state, and not fully adapted to the newer human conditions imposed by the erect position and the artificialities of civilization.

EUGENE MURRAY AARON.

Philadelphia, March 6.

BOOK-REVIEWS.

Die Zukunft des Silbers. By EDUARD SUESS. Vienna and Leipsic, Braumüller. 1892 227 p.

DR. SUESS is eminent as a geologist, and it would be impertinent on the part of the present writer to attempt a criticism, or even an exposition, of his views on the geological and metallurgical conditions which affect the production of the precious metals. Dr. Suess's conclusions are similar to those which he gave to the world some fifteen years ago, in his monograph on the "Future of Gold," published in 1877. He believes that the production of gold is likely to be limited in the future, and will not supply sufficient gold to meet the monetary consumption and the consumption in the arts. He believes also that the production of silver will not progress as rapidly, or that its depreciation will descend as far, as is often supposed. He believes that gold must eventually cease to be used as a standard of value; while the production of silver is likely to continue at a comparatively equable pace, making that metal eventually the basis of the world's money. International bimetallism, even if it were practicable, would be only a half-way measure, paving the way to the ultimate adoption of the single silver standard.

To this line of reasoning, the economist who, like the present writer, believes that the gold standard works to reasonable satisfaction, would answer in some such fashion as this. If it were true that all exchanges were effected by the actual use of coined money, undoubtedly the monetary supply of gold would not suffice. at the present range of prices; and on that supposition the maintenance of the gold standard must be accompanied by a fall in prices, which would in many ways be distressing. But the fact is that in modern communities gold is used but to an insignificant extent as a medium of exchange. The great bulk of the exchanges are effected by credit substitutes of various sorts. Much the most effective of these is the modern machinery of banking, by means of which, especially in countries like the United States and England, an enormous volume of transactions is settled with an insignificant use of coin. So far as retail transactions are concerned, bank notes, government notes, silver as a subsidiary coin, do the greater part of the money work in all civilized communities. Gold, therefore, acts in the main simply as a measure of value or a standard of value; something in terms of which the values of commodities are expressed, and into which all other forms of currency are convertible. It performs its function very largely by being held as a reserve in the great central depositaries, serving simply to sustain and regulate the circulating medium. The evidence does not indicate that the supply of gold is insufficient for this purpose. On the contrary, large accumulations of gold have been made in recent years by civilized countries; by Germany in 1873, by the United States in 1879, by Italy in 1883, by Austria in 1892-3, without causing, in the opinion of the present writer, any appreciable difficulties. It is not impossible that in the distant future the supply of gold will prove insufficient, and that some change may be made by the great civilized countries in their standard of value. But such a change for the visible future is highly improbable. The drift of the time is toward the gold standard in all the great countries; with a constant development and use of credit substitutes, but with gold as the sole basis. So far as we can see into the future, this policy will work no harm, and will conduce greatly to stability and convenience in the circulating medium.

So far as silver is concerned, it is undoubtedly true that the method of occurrence of silver ores makes it probable that each individual find will soon be exhausted. The great bonanzas, of which the Comstock lode was the first in the United States, have soon given out, and the great and rapid increase in the production of silver has been due to successive lucky finds. Geologically speaking, therefore, the enormous increase in production, which has taken place in the last twenty-five years, may be regarded as temporary. But historically speaking, it is impossible to say that these finds will not continue for a period of great length in human history. The hard fact is that the production of silver has increased with extraordinary rapidity in the last twenty years, and that as yet there are no signs of relaxation. If this process continues, the decline in the value of silver cannot be checked. If it ceases, the price of silver in terms of gold is likely, at best, to remain where it now is. In either case, there is no ground for supposing that silver will come to be used on the same terms as gold by civilized nations, still less that it is likely to displace gold, as Dr. Suess predicts.

F. W. TAUSSIG.

Harvard University, Cambridge, Mass.

How to Manage the Dynamo. By S. R. BOTTONE. New York, Macmillan & Co.

THIS little book is meant, as its author tells us, for steam engineers who are called upon to take care of dynamos, without having any previous training or knowledge. As this class is a rather large one, there is no doubt but that there will be a considerable demand.

The book is very clearly written, and contains just about all that the men for whose benefit the author is writing will require

to know. There are some sins of omission and commission, however. Among the former may be mentioned the fact that the alternating-current dynamo is not touched upon, or very briefly. No instructions are given as to what should be done in case, when a dynamo arrives, it is found to be connected for running in the opposite way to which the foundations, etc., necessitate its turning. Instructions for reversing the connections of a series machine when it fails to start up or under compounds would also be of use. The writer has had on many occasions to travel several hundred miles to remedy these simple troubles. A warning about the necessity and method of keeping armatures and fields free from moisture when lying boxed up, might also be added with benefit. Among the latter may be mentioned the direction to lay an armature on waste (page 22), as the latter is often full of pieces of iron, etc., which might ruin the insulation. Sand-paper is preferable to emery for polishing of commutators, as the latter frequently contains particles of iron. The remedy proposed on page 30 for a shunt dynamo which will not pick up is impracticable.

Altogether, this is a very useful and clear little book.

R. A. F.

Electrical Experiments. By G. E. BONNEY. New York, Whittaker & Co.

THIS book is a collection of simple experiments with magnets, induction coils, influence machines, and plating baths Mr. Bonney is already favorably known through his books on the making of induction coils, electroplating, etc., and this volume is quite up to the others

Manual of Irrigation Engineering. By H. M. WILSON, C.E. New York, J. Wiley & Sons. 1893. 351 p. 8°. $4.

WHILE text-books and elementary treatises on the general subject are threatening to flood the market and the profession, the promise of an overplus of good treatises on special branches of engineering, by competent specialists, is by no means serious. Good works of the latter class are always welcome to the average practitioner, and this seems to be one of the kind which is likely to prove both useful and welcome. It is written by an author who has had experience in America, Europe, and India, and contains the fruits of both original investigation and discreet compilation. The book is prepared mainly with reference to the needs of the engineer having charge of work of this kind in the western portion of the United States, and includes accounts of the current methods there in use, as well as of such systems observed abroad as are most likely to prove useful in this country. The collection and distribution of water, but not its application to crops and to its minor uses, constitute the subject chosen for treatment. Much new material is here published, especially relating to earth-dams and elevation of water by pumping The author makes application, in a very sensible manner, of the principle, too little recognized by writers on engineering subjects, that, while no hesitation should ever be felt in regard to the use of mathematics in the development of the subject in hand, its use should always be confined to the minimum quantity, and the most elementary methods, consistent with the effective accomplishment of the purpose in view. The average reader, even though a professional and a practitioner, does not purchase his library with the view of admiring the scholarship, the pedantry, or even the genius of authors.

The importance of this subject may be realized when it is stated, as by this writer, that 25,000,000 of acres are made fruitful in India alone by irrigation; in Egypt there are about 6,000,-000, and in Europe about 5,000,000 acres. In the United States, where this process of conquering nature has but just begun, are now about 4,000,000 acres of irrigated lands. Thus, about 40,000,000 acres of soil are made to produce crops: land which would otherwise have remained desert.

The book is well and freely illustrated, and its typography is that always seen in the technical works of its publishers. It is

CALENDAR OF SOCIETIES.

Biological Society, Washington.

Mar. 11.—Frank Baker, Recent Discoveries in the Nervous System; Vernon Bailey, The Burrow of the Five-Toed Kangaroo-Rat; E. M. Hasbrouck, The Breeding of the Bald Eagle near Mount Vernon (with exhibition of eggs).

New York Academy of Sciences, Biological Section.

Feb. 13.—A paper on the "Functions of the Internal Ear" was presented by Dr. F. S. Lee, based upon study of dog-fish. The results of experiments were given, showing that the semi-circular canals are sensory organs for dynamical (rotational) equilibrium, otolithic parts for statical (resting) equilibrium. Each canal appreciates movement in its own plane, and by a definite functional combination of canals all possible rotational movements are mediated. This theory explains compensating movements of eyes, fins, and trunk. The method of experiment was that of sectioning the branches of the acoustic nerve and stimulation (by rotational movements) of the swimming fish. In a paper by Bashford Dean, on the Marine Laboratories of Europe, a series of views were shown of the stations of Naples, Banyuls, Roscoff, Plymouth, Arcachon, the Helder, and St. Andrew's. H. F. Osborn described the foot of Artionyx, the new member of the order Ancylopoda Cope. It is distinguished from Chalicotherium by the character of aucle and pes, which present a marked resemblance to the Artiodactyla, while Chalicotherium represents these structures as found in Perissodactyla. Both genera are ungulate in aucle joint, but the phalanges terminate in claws, and, in view of the double parallelism between these two forms and the two subdivisions of Ungulates, it was suggested to divide the Ancylopoda into the Artionychia and Perissonychia.

Society of Natural History, Boston.

Mar. 15.—H. C. Ernst, Cultures of a New Pathogenic Bacillus, Illustrating Methods of Isolation; Warren Upham, Deflected Glacial Striæ in Somerville, Notes on the Tertiary Strata of the Fishing Banks Between Cape Cod and Newfoundland.

handy in form and weight, and, in our opinion, will be found useful to all engineers engaged, or likely to be engaged, in any kind of hydraulic work.

AMONG THE PUBLISHERS.

OF the International Education Series published by the Appletons more than twenty volumes have now been issued, one of the latest of which is "Rousseau's Emile," abridged, translated, and annotated by William H. Payne of the University of Nashville. It is not a mere series of extracts, but a judicious condensation, forming a continuous work and giving as much of the original as readers of our time are likely to care for. The important and wide-reaching influence of Rousseau's work has been due in the main to his perception of the grand truth, previously too little regarded, that the child's faculties have a certain natural course of development, and that, if education is to be successful, it must be in harmony with that development. Unfortunately, he knew but little of what that course of development really is, and his practical plans for meeting it were about as inappropriate as they well could have been; and whoever should adopt them would find in the end, if not sooner, that he had followed anything but nature. Rousseau's notions that the child ought not to do anything against his will, that children have and can have no morality, and that all wickedness is weakness, are both false and mischievous; and many others of like character might be cited. Moreover, though a vehement democrat in politics, he would refuse the benefit of education to the poor, expressly saying that "the poor man has no need of an education," and he held that women ought to be educated merely to please men. Mr. Payne is clearly conscious of these faults in Rousseau's work, and sharply animadverts on some of them in the short but very suggestive notes that he has furnished to this volume. Moreover, he does not hesitate to attack some of the educational fads of the time; and his comments add much to the value of the book. Indeed, we think the public would be glad to receive from him an independent work of his own, in which his views might be stated more at large.

— Charles Scribner's Sons have in preparation "How to Know the Wild Flowers," by Mrs. William Starr Dana, with 100 illustration by Marion Satterlee.

— The latest issue in Scribner's series on the Great Educators treats of "Froebel and Education by Self-activity." The author is an Englishman, Mr. H. Courthope Bowen, who is an enthusiastic disciple of Froebel, and has had much practical experience of kindergarten work. We cannot say, however, that his book is a quite satisfactory treatment of its theme, the literary form of it being in some respects defective. There is a good deal of repetition in it, as indeed the author himself admits, and the sentences are often loaded down with parenthetic expressions which make them awkward and sometimes obscure. The first two chapters relate the principal events of Froebel's life, the processes of his own education and his various experiences and experiments as a teacher. Then follows a notice of his philosophy, which, however, Mr. Bowen makes no more intelligible than others have done, and then an exposition of his theory of education. The remainder of the book is devoted to a description of the kindergarten and other contrivances that Froebel designed, with some account of his relation to earlier and later educators, thus giving on the whole as full an exposition of his views and methods as most teachers will desire. As to the value of those methods themselves, we have not space to speak largely; but we cannot help thinking that both Froebel and Pestalozzi are at the present day greatly overrated. Their methods are only adapted to a few years of early childhood, and are not perfect even for that period; while their prejudice against book-learning was little short of barbarous. Nevertheless, whatever is good in their systems we want, and we trust that our teachers will not fail to appropriate it.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale—A Zentmayer new model U. S. Army Hospital monocular stand. Price $110, will sell for $75. Address H. C. Wells, No. 151 Broadway, New York.

For sale—A complete set of the third series of the American Journal of Science (1870-1898) handsomely bound in single volumes in dark brown half morocco. Address G. H. Williams, 808 Cathedral Street, Baltimore, Md.

For sale, or for exchange for books on medicine or surgery, new editions only, a large geological library, containing nearly all the State and Government Reports since 1855. Will be pleased to answer letters of inquiry and give information. Address R. ELLSWORTH CALL, Louisville, Ky.

For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanical stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange.—I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

Wants

WANTED—Second-hand copy Tryon's Systematic Conchology. Please quote condition, date, price. H. L. Osborn, Hamline University, St. Paul, Minn.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.